

VALENTIN'S QUALITATIVE ANALYSIS

A Course of Qualitative Chemical Analysis. By William George Valentin, F.C.S., Principal Demonstrator of Practical Chemistry in the Royal School of Mines and Science Training Schools, South Kensington. (London: J. and A. Churchill. 1873.)

IT is a good sign of the present activity of scientific study in this country that there should have already been a call for a second edition of a work which only appeared two years back, in the early part of 1871.

The author has, in the second edition, separated the second part of his original work, and this, treating entirely of qualitative analysis, forms the volume now before us. The elements which occur in the main as bases are divided into five groups, and the first portion of the book is devoted to a careful study of each element of each group beginning with group V., a method the advantages of which will be seen by a very short study. The first 103 pages are devoted to this matter, and the attention of the student is then devoted to the study of the reactions of the acids. No particular grouping is here attempted, the acids being simply taken under the head of the principal element of each, *e.g.* sulphuric acid is followed by sulphurous acid, and that by hyposulphurous and hydrosulphuric acids. We remark here, by the way, that the polythionic acids are dismissed with the notice that they must be reserved for a more extensive course of study. A few of the more common organic acids are then referred to, and the whole matter treated of is shown in the condensed form as tables. In these we notice no important alterations from those of the edition of 1871, and of them we can, after considerable experience, speak in the highest terms, students soon learning to use them with great accuracy and despatch.

Mr. Valentin has stated in his preface that he purposely omits considering the rarer elements in his tables. In this we cordially agree with him as regards the tables intended for students, but we cannot help wishing that Mr. Valentin had put in the appendix some analytical information with regard to these bodies in a tabular form; as we feel sure that his great experience in the analysis of every possible kind of body would have enabled him to give valuable information to many who are compelled occasionally to make diligent search for elements which are not always met with. Many old students of the College of Chemistry will recognise an old friend on pp. 50 and 51 in the alternative table for group IIIA., it being no other than the old table used there up to the time of the introduction of the newer methods given at the end of the book.

We notice with pleasure that the analytical tables are published in a separate form, printed on De La Rue's parchment paper; this is certainly very good news for chemical students who have to use them. Who does not know the gradual process of obliteration and destruction by acids and alkalis which gradually, but surely, rendered his most carefully prepared and written analytical tables useless. It would be a great boon to all compelled to use books in the laboratory, if some modification of this material could be used for binding them. In conclusion we can strongly recommend the book to anyone desiring either to get or to give

a thorough grounding in analytical chemistry; and the only fault we can find with it is that rather too profuse use is made of symbolical formulæ, for they are scarcely required in a book on analytical subjects only, and the first volume gives quite a sufficient amount of information on their use and nature. We hope that Mr. Valentin will some day give us a quantitative analysis.

R. J. F.

OUR BOOK SHELF

Celestial Objects for Common Telescopes. By the Rev. T. W. Webb, M.A., F.R.A.S. Third edition, revised and enlarged. (London: Longmans, Green, and Co, 1873.)

POSSESSORS of what Mr. Webb calls "common telescopes," will be pleased to have another edition of this most useful adjunct to their instruments, with corrections and additions up to the present time. Now that silvered glass reflectors are so cheap, and apertures little below six inches not uncommon in the hands of amateur astronomers, the author's definition of a common telescope is probably too limited, but these limits are extended as we proceed with the book and find mention of objects barely visible with nine inches. The advice on the use of telescopes, and the mode of observation is sound and good, and too much stress cannot be laid on the necessity of a good solid stand; a good telescope will be absolutely useless with an unsteady mounting. The description of the various phenomena to be viewed in the members of the Solar system may lead possessors of small telescopes to expect too much, the separation of Saturn's rings, the markings on Jupiter's satellites, to wit, although mention is made of the apertures required to view the features mentioned; but this may also make the book useful for work with larger instruments. We must take objection to the great contrast of light and shade, as is often the case in other works, in the cuts of Venus and Jupiter's moons, the dark markings on Venus being infinitely too black, they in reality being only just visible, with first-rate instruments, to a practised astronomer. Drawings of this kind only represent position and shape, but it must be remembered that an amateur expects to see through the telescope exactly what he sees in a drawing. One-third of the book is taken up with a selection of double stars and nebula, as in the former editions, with measures of position and distance up to later dates. Altogether the book will be found most useful to every incipient astronomer, but perhaps there may be too strong a tendency to star-gazing induced by it, and we should have been more gratified to have seen directions to readers having telescopes of certain sizes how to make their observations of real use and not a mere pastime. For instance, double image micrometers can be used on less apertures than 6-in. without clockwork; and some instructions in the use of them, and in reducing their observations so as to show the motions of binaries, would be of great service in teaching amateurs to do useful work; a hint, also, on drawing the ever-changing belts of Jupiter, any extraordinary spots on the sun, the larger nebulae, and last, not least, the star clusters. As soon as amateurs have seen the planets and a few double stars, they should begin to make themselves useful, otherwise they soon get tired of the mere star-gazing and the telescope becomes to them a thing of the past.

G. M. S.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

Dr. Sanderson's Experiments and Archebiosis

IN last week's NATURE Dr. Sanderson expresses some surprise that I was gratified by the facts recorded in his previous

letter. My reasons were these. Dr. Sanderson's experiments in the eight successive cases in which he employed the temperature of 100° C. for twenty minutes were entirely confirmatory of my own, and were, moreover, so conducted as to refute the objections which have been urged by Dr. Wm. Roberts and others.

As to the bearing of Dr. Sanderson's experiments with higher temperatures and more prolonged periods of exposure to heat upon the general question of the independent origin of living matter, I wholly dissent from his now expressed conclusions, for the following reasons:—

In the first place his fluids were not kept sufficiently long before they were submitted to microscopical examination. Dr. Sanderson is quite mistaken in supposing that in examining his liquids within 3–6 days after their preparation he was following my method—more especially in cases such as these where the fluids have been exposed to temperatures higher than usual, or to 100° C. for upwards of twenty minutes. Three to six weeks have often elapsed before I thought it judicious to open my flasks (See "Beginnings of Life," vol. i. p. 355, p. 441, and Append. C.). In opening all his flasks at the end of 3–6 days, Dr. Sanderson lost the opportunity of watching the changes which might have ensued later in many of his experimental fluids—and hence lost his right to draw any conclusions from these abortive trials.

Secondly these experiments are open to another objection. Dr. Sanderson concludes from them that exposure to a temperature of 101° C. almost always arrests the tendency to fermentation in his experimental fluids. This conclusion I believe to be erroneous, because in the former series of experiments which I performed in his presence, and of which he recorded the results in your pages (NATURE, vol. vii. p. 180), fermentation occurred in the majority of cases in fluids which I have very good reasons for believing to have been raised to a temperature of 103.33° C.* The method recently employed by Dr. Sanderson for superheating his flasks was needlessly complicated, and the exact temperature to which they had been exposed was known only by inference—never by direct thermometric observation.

Leaving now the discussion of the experimental facts I come to the examination of Dr. Sanderson's inferences, which seem still more open to objection.

Dr. Sanderson, in common with most others, had up to the date of his witnessing my experiments, admitted that Bacteria and their germs were killed in all fluids with which he had experimented at the temperature of 100° C. (see "Thirteenth Report of Medical Officer of Privy Council, 1871.") It was, indeed, this conviction which inspired himself, and many others, with a strong disbelief in the results which I obtained with previously boiled infusions.

What remains, then, for Dr. Sanderson to do, prior to drawing inferences such as he now expresses, is to ascertain, by direct examination, whether the temperature of 100° C. is or is not fatal to the life of Bacteria. It is upon this that the interpretation of my results can alone depend. I have already contributed my share to the inquiry by several long series of experiments, each of which has led me to the same conclusion, viz., that Bacteria and their germs, when in the moist state, are killed at a temperature of 60° C. (See "Beginnings of Life," vol. i. p. 325–333; "Proceedings of Royal Society," No. 143, 1873; and another paper about to appear in the next number of the "Proceedings.") It is for Dr. Sanderson, or any competent observers who are sufficiently interested, to examine my experiments and results on this part of the subject, or else to devise others for themselves having a similar bearing.

If I am right in believing that 60° C. is the thermal death-point of Bacteria in the moist state, the conclusion which must be drawn from the now admitted results occurring in fluids which

* Dr. Sanderson was not aware of this fact, and says he does not know any means by which the temperature of a fluid boiling briskly in a vessel from which the steam escapes only through a capillary orifice, could be accurately estimated. The method which I adopted some months ago seems to possess this merit. I had a small maximum thermometer made for the purpose, 2½ in. in length, and graduated from 95°–115° C. Having straightened the neck of one of my retorts (capable of holding about two fluid ounces), it was filled with some hay infusion and the thermometer was introduced in such a way that its bulb remained in the midst of the fluid, about three quarters of an inch away from the glass. The long neck of the retort having then been drawn out and broken off (so as to leave the usual capillary orifice) the fluid was boiled for five minutes before the vessel was sealed. The thermometer was found to stand at 103.33° C. The retorts employed in my previous experiments with Dr. Sanderson were of the same size, and their contained fluids were boiled under precisely similar conditions. If larger flasks, containing more fluid, were employed the temperature would doubtless rise to a still higher degree owing to a corresponding increase in internal pressure.

have been heated to 100° C. suffice for my argument as to the reality of Archebiosis. The further investigation of the results of raising fluids to higher temperatures for protracted periods is of great interest, but does not at all affect the question of the reality of Archebiosis; and Dr. Sanderson's present experiments have, therefore, none of the significance in the argument which he strangely enough appears to claim for them.

Briefly, having admitted that Bacteria arise in fluids which have been submitted to a temperature of 100° C., it is for Dr. Sanderson to show that they are not killed in fluids at 60° C., as I maintain that they are, before he can attempt with any effect to draw inferences of his own, or to criticise those which I have drawn on the subject of the independent origin of living matter.

H. CHARLTON BASTIAN

University College, July 7

Dr. Bastian's Experiments

REGARDING Dr. Bastian's letter in NATURE of June 26, I am happy to be able to make a note of an experiment which is of interest and importance. I sealed a tube on to a flask of about 100 cc. capacity at right angles to the neck, and drew out the end so as to form a capillary orifice. About 30 cc. of water were put into the flask, and a thermometer in an india-rubber cork was wired into the neck. On boiling the water the steam had not issued during more than half-a-minute, before the temperature was 102° C., and in less than ten minutes it had reached 118° C.; fearing the safety of the apparatus, I did not proceed further, nor indeed did I wish to do more. The joint experiments of Drs. Sanderson and Bastian, then newly published in your paper, led me to this. My view being that Pasteur's experiments on milk, mixed with carbonate of lime, and the liquid known as "Pasteur's solution" mixed with carbonate of lime, conclusively show that liquids which ordinarily develop Bacteria, will, if they remain neutral after boiling at 100° C. also develop these organisms: raise the temperature to 110° C. and the Bacteria no longer show themselves.

Thus believing, I concluded that the absence of Bacteria in some of Drs. Sanderson and Bastian's flasks in which were placed neutral or only slightly alkaline infusions, was probably due to the liquids being heated above 100° C., by boiling in vessels with capillary orifices. That my supposition was correct is more than likely; in fact experiments with infusions confirmed it. That an aqueous solution may so easily be raised to 118° C. is a point in chemical manipulation which will be turned to advantage in the laboratory.

King's College, June 30

WALTER NOEL HARTLEY

Temperature and Pressure

THE climate of the island of Jamaica is remarkably uniform, not only at the sea-level, but also at places having the same elevation, so that the connection between temperature and elevation, or barometrical pressure due to that elevation, is easily obtained; and since the surface of the island is broken up by innumerable radiating and intersecting mountain ranges, among or upon which the houses are scattered, this connection becomes one of the most important features in its meteorology; but what renders it especially interesting, however, is the fact that the rate of the decrease of temperature in ascending the hills in this tropical climate is equal to the average rate of decrease found by balloon ascents made in England, as far as the irregularities of the results obtained from those ascents will allow us to judge.

In order to show that this is the case, let t_0 be the temperature at any place where the pressure is p_0 , the temperature being expressed in degrees of Fahrenheit's scale, and the pressure in inches of mercury at 32°; let t and p be the corresponding quantities at any other place above the former; then if λ be constant and equal to 3°·23, the equation

$$t_0 - t = \lambda(p_0 - p)$$

will represent the connection between temperature and pressure; or in words, for every inch the barometer may fall, the thermometer will fall 3°·23.

If we take mean annual values, at Kingston $t_0 = 78°·8$, $p_0 = 29·97$ in.; and at Newcastle, the garrison of the white troops, $t = 67°·0$, $p = 26·31$ in.; so that $\lambda(p_0 - p) = 11°·8$, which is exactly equal to the observed difference of temperature.

Again at Craighton, the residence of his Excellency the